November 4, 1952

Dear Bruce:

Perhaps I should have waited a few hours to post my last letter. Yours of Oct. 30 arrived yesterday afternoon. I am sorry that my misaddressed letter never reached you. Enclosed is my carbon copy; I have a photocopy of it, I suppose the original will eventually return.

The notion of "hold-fast" is worth consideration-- we had speculated before that E. coli lysogenicity involved the synapsis of a transduced Lp locus along with lambda, i.e., that the induction of lysogenicity is, in a sense, another transduction. The Vi-phage story sounds something like phenotypic mixing (Hershey; Novick and Szilard). Anderson does not seem to be acquainted with this work.

I am glad to hear that you are giving some priority to writing up the O work. The f-2 tests are now complete, as expected, the FA from a second-generation i transmits both b and i to SW-543. I don't think that I will, after all, bother with this same analysis with another factor, The first generation seems to be valid on the face of it. Re serological analysis, the b-agglutinability of SW-543 is still paradoxical. The cultures showing it give a large proportion of rough colonies. These, however, give a perfectly non-specific agglutination, while the broth from which they came still reacts quite specifically with b. Perhaps there is some sort of unmasking of a latent b antigen in the incipient rough O form. FA(666/588) finally gave a swarm, but the count is about 30 tracks per swarm. SW-666 + SW-534 give a nice demonstration of O + O cells giving a swarm, but strangely enough this has been 1,2 Gal+ , indicating that the SW-534 was the transducee. Conceivably there is another phage (from 534 growing on 666), or the initial output from 666 is remarkably high.

Boyd has sent all his lysogenic typhimurium, but these have not arrived. Let us keep in tough about these phages. Spicer's phages don't seem to be doing very much yet; by all means let us have any group O forms, at your convenience.

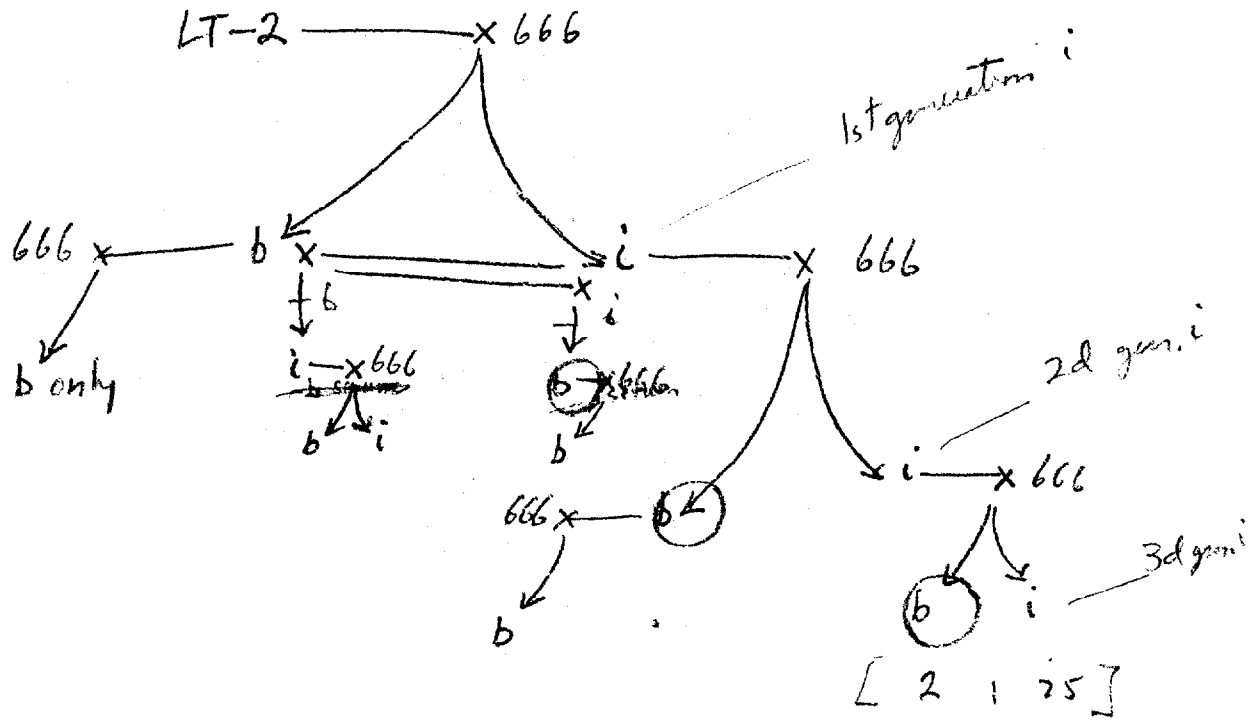
I am especially interested in the possibilities that may come out of your para A Oform. If it also shows a linked transduction, it should be also linked to the 543 factor. But it's not likely to be identical, since the 543 transinductions are usually FA type, much less frequently b. (My data on the proportions, even for comparable experiments, are not homogeneous. There may be some interesting environmental controls to be examined.) Have you studied the allelism of the paraA O with 543? If the linked transduction is not a fluke (reversion?) it may be possible to orient the three postulated factors-- I'd like to look at this unless you're already busy with it.

I'll be happy to send these other transductions to SW-543, but I don't really see the necessity of a detailed serological study on all of them. The O form, the spontaneous and induced b's and i's certainly should be. I have working arrangement with Edwards for fairly full verification of sero-types, but if you think it should be done, let me know, Instead, how about

the suspicious b and j forms that have come up. I'll send you the lot, together with 665 and 666. Before I make up the package, let me know if there's anything else you're likely to need.

Selection with Chi phage is far more efficient in liquid medium (ca. 1000-10,000 fold). With the addition of a known number of marked, reference O cells, it may be feasible after all to look at the flares more selectively. Also, selection against H by antiserum looks much more encouraging now that the unexpected b-agglutinability of the 666 culture has been discounted.

The final pedigree:



The tests controverting the hypothesis of 2 separate genes are O
Some less pertinent points are omitted. The 3rd gen. was not crucial
but seemed a good idea